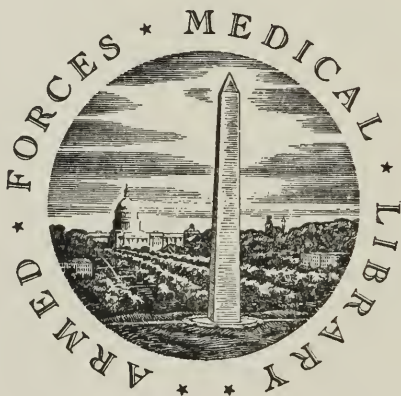


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A DISSERTATION

ON

DROPSY.

Submitted to the Examination of John Andrews, D. D. Provost, the
Trustees and Medical Professors, of the University of Penn-
sylvania, on the 25th of April, 1811,

FOR THE DEGREE OF DOCTOR OF MEDICINE

BY HENRY CHAMBERS,

OF VIRGINIA.

Honorary Member of the Medical Society of Philadelphia.

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TO
JOHN R. LUCAS, M. D.
OF MECKLENBURG, VIRGINIA.

THE publication of this Dissertation might well have been dispensed with. But my feelings and duty demanded of me some public testimonial of my gratitude to you for the many great and friendly services, together with the invaluable private instructions I received from you, while your pupil. Accept of this, not as a recompense, but as a manifestation of the grateful sense I have of your favours.

That you may long continue to pursue that profession to which you have done so much honour, and enjoy the domestic happiness with which Providence has so liberally blessed you, is the most cordial wish of your friend and pupil,

THE AUTHOR.

TO
GEORGE CRAIG, Esq.
OF LUNENBURG, VIRGINIA.

BLESSED by nature with the first rate talents, and highly cultivated by education, it has long been a serious cause of regret to your friends and acquaintances, that you have not before this taken a more active part in the service of your country; whose situation has so much required it. I am happy to hear you have at length come forward, and I sanguinely anticipate that before long you will be one of its brightest ornaments.

Accept this humble tribute of respect from your friend,

THE AUTHOR.

A DISSERTATION

ON

DROPSY.

A FEW years ago this disease was justly termed one of the *opprobria medicinæ*. But a new era has appeared. No longer does the physician approach the hydropic patient with fear and despondence, conscious of his skill being baffled.

Nor is this alone the case with dropsy: the same doctrine, which raised the veil from this, has directed our before wayward and uncertain efforts in opposing other diseases of the same terrific aspect. I mean the doctrine of the unity of disease.

Before this was taught, the practice of physicians had been entirely empirical: like the mariner borne on the stormy deep without needle or compass to direct him. Whence sprung the great diversity among physicians in treating diseases. Whence the success which attended the different, and even opposite modes of treating the same disease. They were all right and all wrong. From these circumstances,

doubt began to be entertained of the power of medicine. For what could be inferred from these apparent contradictions? Either that opposite causes would produce identity in effect; or that nature had a prescribed mode of curing disease, which overleaped ordinary medical power.

Happily for mankind, the doctrine of the unity of disease has solved the difficulty, and restored thousands to health and happiness, who otherwise would have been immolated on the altar of ignorance.

That dropsy depends on increased effusion or diminished absorption is admitted by every one; and that this depends on an increased action in the arterial system, in all cases of general dropsy, shall be the object of the following dissertation to prove.

In delivering this opinion, I am conscious I differ from all authors on the subject. Even the immortal author of the doctrine of the unity of disease, whose observations on dropsy stand unrivalled, believes there are some cases which are not preceded by high arterial action. I would not be understood to assert that this exists in every stage of dropsy; for I am certain in many cases, after the dropsical effusion has taken place, great debility is induced in the whole system: which is nothing more than we find in other violent affections, in which spontaneous evacuations occur. Thus arterial action is reduced

by hemorrhages, by sweats, and by evacuations from the bowels. And why should not the same thing occur from hydropic effusions?

Debility is no doubt the predisposing cause of dropsy; and that this should give rise to a disease of high morbid action is manifest from the course of all diseases: and that high arterial action should exist in one when obvious debility pervades every other system alike accords with the observation of every intelligent physician.

Indeed it seems to be a law of the animal economy, that when high action exists in one system, a minus grade should be induced in the others.

Having premised thus much, I proceed to mention some of the most obvious causes of dropsy.

In a healthy person, a watery or serous fluid is constantly exhaled into every cavity of the body; and this without being accumulated is immediately absorbed by a set of vessels adapted for the purpose. Whatever increases this exhalation or prevents absorption must produce dropsy.

It has not been sufficiently attended to by physicians what effect high arterial action has in producing dropsy. Nor can there ever be a rational or successful method of practice, in this form of disease, until this be attentively regarded.

We find it occurring at the close of all fevers

of high morbid action in which bloodletting has not been used, or used too sparingly.

That it only takes place under these circumstances in fevers, I infer from its never occurring when depletion has been freely employed, or in those of low morbid action, from the beginning. That in this case it depends on increased excitement in the arterial system is very evident. But how it should be produced by it in some other cases is not so easily explained.

Dropsy often occurs in women either from obstruction or retention of the menstrual discharge; and this we know is often productive of the highest grade of morbid excitement: and may it not be, that the dropsical effusion only takes place when some increased action does exist? It may be asked, if this be a fact, why does not dropsy occur in all cases of increased arterial action? To this I answer, that preternatural action may exist, and not rise to the point at which dropsical effusions take place; or it may transcend it: as we often see morbid action either not rising to, or transcending, the point of inflammation.

It is sometimes produced by excessive evacuations; and indeed all the causes which produce obvious debility in the system.

I very much regret my never having seen a case

of this description; for I have no doubt observation would prove, that high arterial action existed even here before the dropsical effusion took place.

This, to some, may appear paradoxical; yet it is easily explained. Thus persons who fast a long time are seized with a violent fever; the abstraction of the stimulus of food producing direct debility; and of course the system becomes more excitable: on the same principle is the former position explicable.

It is certainly preposterous to undertake to prove the physical possibility of high morbid action originating from debility; for I believe no physician of the present day doubts debility being the predisposing cause of all forms of disease. From this it is evident, the above position is in perfect consistence with the common and established laws of nature.

Reasoning from cause to effect is certainly one of the fairest ways of ascertaining truth; for, *cæteris paribus*, identity of cause will produce identity of effect. And reasoning from analogy, though sometimes fallacious, is often correct. Thus we see gout and rheumatism produce serous effusions in the joints. This never occurs when low morbid action exists. In many diseases we find the pulse reduced by sweats; the same thing takes from dropsical effusion; but in many of violent action, we observe sweats occur without effecting any reduction of the

arterial action. The same thing takes place in the hydropic state of fever; for we often find high action after the effusion of water: all of which we find in states of disease in which spontaneous hemorrhages occur. In fact, it is all a unit, whether morbid excitement be reduced by sweats, by spontaneous hemorrhages, or by dropsical effusions.

Dropsy often arises from obstructions of the viscera. From analogy, I am forced to believe that either local or general morbid excitement must exist even here previously to the dropsical effusion. Thus obstructions of the liver, as often as any others, produce it; and these, we know, often assume the form of an active disease.

In fact, as soon should I expect a discharge from the skin by an application which would not stimulate it, as dropsy not preceded by high arterial action.

Dropsy is no disease: it is only the effect of disease. With the same propriety could we say that the lymph, which lines the trachea in cynanche trachealis, is the *morbis ipse*, as the effusion of water in any cavity of the body constitutes the disease itself. I have mentioned it as a disease only in obedience to custom, and for sake of perspicuity.

Small indeed has been my opportunity of deriving any benefit from experience. My knowledge has

necessarily been drawn from books: and with regret I assert, until lately none have described, with sufficient precision, the symptoms accompanying this disease. But a few years ago, a physician would have been ridiculed, who would have dared to feel the pulse of an hydropic patient: he would probably have been stigmatized as an empiric or a fool.

But the new world has been peculiarly blessed. Liberated from the iron grasp of despotism, we ventured to think for ourselves, and forever turned our backs on the absurd dogmas of the old schools.

Nullius addictus jurare in verba magistri, was the motto. Ages to come will venerate the name of him, who first threw off the shackles and taught physicians how to think.

Having, in a slight and succinct manner, considered some of the most obvious causes of dropsy, and endeavoured to prove it is always preceded by high arterial action, it remains for me to say something of the method of cure; from which I think the principles I have advanced will derive additional support. Of this it is not my intention to treat at length. I shall not descend to minutiae, but only speak of general indications.

A case, which came under my observation some short time since, I think deserves to be noticed here; particularly as it is decidedly in favour of the opinion I have advanced.

A servant about thirty-five years of age, after a violent fever was taken with a general dropsy.

His abdomen was very much distended; and he was completely anasarcaous. In defiance of all the medical aid which could be obtained, his disease continued to increase. Finally the physicians determined it was useless to persevere. At this time the difficulty of breathing was prodigious, and he was compelled to remain erect to prevent suffocation. In fact every symptom marked impending dissolution. His fears and despondence were so great as to prevent his taking any kind of nourishment: and now, together with the distress occasioned by the water, danger was to be apprehended from his extreme debility. In a very short time all were astonished at the rapid increase in his discharge of urine. Instead of the red and feverish appearance it had formerly exhibited, it was now perfectly natural and in large quantity. From this time his recovery was rapid and perfect. I should observe no depleting remedies were used, (diuretics excepted), and tonics and stimulants were for the most part relied on. In this case the *rationale* is evident. High morbid action doubtless existed. And as long as the medicines before mentioned were given they only served to increase the disease; but as soon as they were omitted the sedative effect of fear cooperating

reduced the morbid excitement of the bloodvessels, and absorption took place. No doubt, had depleting remedies been used the disease would have been conquered at a much earlier period. In which assertion I am warranted by subsequent facts, which almost incontestably prove it. The same man after three years interval was taken with a violent fever. I had an opportunity of seeing him: and, being apprised of his old complaint, I examined and found his lower extremities completely edematous. His head was considerably affected; and his pulse synocha. It was not without much reluctance, his friends permitted the physician to bleed him, from his great debility and emaciation.

His disease continued to increase for several days; but, by strict adherence to the antiphlogistic plan of treatment, in three weeks the swelling subsided; and he perfectly recovered.

In this case, there was the greatest possible degree of obvious debility in the system; and few, I imagine, would have ventured to prescribe bloodletting. But the result proves, beyond the possibility of doubt, the propriety of the plan. May not the edematous swellings, which occur in certain fevers, be the result of nature's efforts to free the system from disease?

Perhaps no form of dropsy will serve better to

elucidate my ideas, on the subject generally, than *hydrocephalus internus*. I hope therefore I shall be excused the following digression.

To give all the symptoms of this proteiform disease, as Dr. Quin has justly termed it, would swell this dissertation to too great length. I will mention a few of the most characteristic, and refer to Dr. Rush's *Inquiries* for the rest.

This disease steals on almost imperceptibly. At first the patient is languid and inactive, often drowsy and peevish. After a while, a sharp headach comes on, together with an intolerance of light, and not unfrequently a dilatation of the pupils. In fact, every symptom manifests a great degree of morbid action existing in the bloodvessels of the *brain*. These symptoms continuing for some time, subject as they always are, to great fluctuation, a very different set make their appearance. The pulse, which, before was slow, intermitting and irregular, now becomes weak, quick and regular. The breathing difficult, nearly resembling stertor apoplecticus. The causes, both direct and indirect, are such as determine a preternatural quantity of blood to the brain. We find it occurring particularly in infancy; from this obvious cause, the brain in children is larger in proportion to other parts of the body than in adults; and of course a greater proportion of blood is sent

to it than in subsequent periods of life. When we consider the seat, causes and symptoms of this disease, we without hesitancy decide, it alone depends on increased action in the arterial system; and that the dropsical effusion is not the disease, but only the effect. In what respect then does this differ from dropsy in other parts of the body? It certainly differs in its seat; but in every other respect, I believe the discrimination to be imaginary. In no disease does the advice, given by the illustrious professor of the practice of medicine, appear more indispensable than in this, viz. to attack disease in its forming state, to eradicate it from the system before it has taken deep roots. Nature dictates this to us; whose directions I fear we too often slight: we too often quit realities to meditate on chimeras, and slight experience to pursue hypothesis and conjecture. If this be not attended to, all our efforts will be of no avail. Few indeed have been cured when the disease has been suffered to form itself; but thousands have been restored to health by attacking it in embryo. A small bleeding or cathartic, in this stage, will more probably succeed, than the most powerful remedies in the former. In addition, I will only remark, that in the first stage only, those remedies can be successfully employed, which are calculated to take down high arterial action: viz. bleeding,

blistering, purging, and cold applications. My time will not admit of my treating particularly of the method of cure in every stage of this disease. For a more minute account of which I refer to Doctor Rush's *Inquiries*.

In examining those authors who have particularly written on the cure of dropsy, one circumstance immediately strikes our attention: That all cases of sudden and profuse evacuations of water, by the urinary organs, uniformly have been preceded by some sedative cause. From this we rationally infer remedies of that description have been too sparingly used.

OF BLOODLETTING IN THE CURE OF DROPSY.

This is certainly one of our most valuable remedies. And if the opinion I have supported be correct, although much used of late, it yet deserves to be more extensively employed: that it is indispensably requisite, when high morbid action exists, no person doubts: but that it is admissible, when only ordinary excitement exists, will not be so readily admitted.

When we consider what has been the uniform effect of large spontaneous evacuations in dropsy, and what has been the result of sudden and direct debility in the system by whatever cause induced in promoting absorption, I think we should be warranted in the experiment.

The case related of Dr. Johnson, by Sir John Hawkins, and noticed by Dr. Rush in his excellent treatise on dropsy, certainly somewhat supports this opinion. Here the fear of death, fasting, and the devotional exercises which engaged his attention, all cooperated in reducing the excitement of his system very low; and absorption took place. Certainly the same result would have been produced much more directly by bloodletting. Dr. Rush mentions several cases, in which he prescribed fasting with manifest advantage. Fear has been found successful from this obvious reason: it is a debilitating passion, and had the effect of reducing morbid excitement. That fear acts in this manner is evidently established, by cases which are related of sudden and copious evacuations of urine just as the surgeon was about to perform paracentesis. Here the excitement was reduced very low; and indeed death, in some instances, has been occasioned. From these facts I again assert that, in my opinion, in many cases the physician would be warranted in making the experiment of inducing great sudden debility in the system by depletion. And, even when preternatural action does not exist in the bloodvessels, I have no doubt but that fasting, fear, and bloodletting, all act alike in dropsy: viz, taking down morbid excitement.

Purges have often been recommended and advantageously employed. Mr. Murray observes a balance is preserved in the system between exhalation and absorption; so that when one is increased the other must be also. The increased secretion and discharge of serous fluid, which cathartics occasion, causes an increased absorption; whence the effused fluid in dropsy is removed. Had Mr. Murray said that this balance exists in the healthy body, doubtless he would have been correct; but in the system, labouring under dropsy, precisely the reverse takes place. For, as long as a balance between exhalation and absorption exists, there can be no dropsy: as exhalation must be in over proportion to absorption to form the disease. Therefore the *modus operandi* of cathartics in dropsy cannot be explained on that principle. I have no doubt they act by producing a diminution of fluids in the system, and of course cause an abstraction of stimuli. From this it appears, they can only be used in those cases which require depletion.

Emetics have frequently been beneficially used in dropsy. The *modus operandi* of these medicines is one of the *arcana naturæ* which time alone can develop. My observations, I think, have proved that absorption takes place in much greater degree when sudden debility is induced, than at any other time.

Then may not the operation of emetics, when absorption is immediately induced by them, (which is often the case), be explained in the following manner: The nausea excited, being a disagreeable sensation, is equivalent to the abstraction of stimuli, and of course brings on direct and sudden debility. From this it must be evident that bloodletting, cathartics, emetics, fear, and fasting, all act alike in dropsy: viz. favouring absorption by taking down morbid excitement.

It remains for me to say something of that class of medicines termed *diuretics*. They are such medicines as are supposed to increase the urinary discharge. They either act directly on the secreting vessels of the kidneys, or by means of sympathy: the action being primarily excited in the stomach and propagated by nervous communication. It is a question of some moment, whether ever very great benefit be derived from the use of these medicines in dropsy. I am very much inclined to think that they seldom, if ever, are powerful remedies in this disease; and that when any of them are usefully employed, they produce their good effects by an action on the whole system. Thus many of the diuretics are of a cooling nature, and others of a tonic; and to these qualities I believe their good effects are to be referred.

I do not mean that they have no specific operation

on the urinary organs; for I have no doubt they have, and are often palliatives. But that they ever produce a radical cure I cannot believe. After the effusion of water has taken place, great debility sometimes occurs with very feeble arterial action. Here tonics and stimulants alone are to be depended on.

I have now, gentlemen, concluded my Inaugural Dissertation. I am fully sensible of its many imperfections; for which my only apology is the short time I have been allowed to compose it. To conclude without returning my most cordial thanks to the professors of medicine in the university of Pennsylvania, for the kindness and attention shown me while a student, and the many opportunities of acquiring useful knowledge which they have severally afforded, would argue the greatest ingratitude.

That happiness and prosperity may attend you all, gentlemen, is my most sincere wish.

To doctor Rush I am particularly indebted. His assiduous attention to me, while confined to a bed of sickness, merits my most grateful acknowledgments.

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